1874MNRAS..35...22L

Hence Struve may have seen *Umbriel* November 1, and December 1847.

In 1871 observations of *Ariel* and *Umbriel* were made at Bothkamp with a telescope of 12-inches aperture, and compared with Marth's ephemeris for that year. The positions of *Ariel* differ from their predicted positions by a large angle, nearly 180°; while the positions of *Umbriel* agree well.

Ariel, however, is much the brighter of the two inner satellites; and as it evidently was not seen at all, it becomes probable that a small star was mistaken for *Umbriel* on the five nights of observation.

This supposition is strengthened when we consider that the Bothkamp observers found *Titania* and *Oberon* difficult objects, which they certainly are not to any telescope which will show *Ariel* or *Umbriel*. Lassell estimates that *Oberon* and *Titania* are twice as bright intrinsically as either of the inner satellites, and this estimate is probably not too high.

We may then fairly claim that Sir William Herschel saw all four of the satellites of *Uranus*; that Lassell discovered independently and first of any of his contemporaries the two faint satellites; while Struve probably saw *Umbriel* on one or two occasions; and that the inner satellites have not all been seen with any telescopes, save the 20- and 40-foot reflectors of Herschel, the telescopes of Mr. Lassell (2- and 4-foot reflectors) and by the Clark refractors at Washington.

The great refracting telescope of Dr. Henry Draper and the Clark refractor of Chicago and Mr. Newall's Cooke refractor have never been used upon these objects so far as I know, although all three are undoubtedly adequate to the research.

1874. *U.S. Naval Observatory, Washington.*

In reference to the foregoing Paper, Mr. Lassell has written to the Secretary of the Society the following letter:—

Dear Sir,—At the Meeting of the Royal Astronomical Society on the 13th inst., when Professor Holden's Paper on the Inner Satellites of *Uranus* was partly read, I took exception to some of its statements, and have in consequence been favoured with the perusal of a copy of it as set up in type. I respectfully request your insertion in the same notice of the following remarks:—

First, I cannot for a moment admit the correctness of Professor Holden's statement, that—"We may conclude then that the elder Herschel was in truth the discoverer of *Ariel* and *Umbriel*, as well as of *Titania* and *Oberon*." From a less distinguished authority than that of an astronomer writing from the Washington Observatory, the assertion might have passed unnoticed, from a conviction that it would not obtain credence;

1874MNRAS..35...22L

but Professor Holden's conclusions will probably be held to have an importance, or prestige, which compels me at some length to prove how perfectly erroneous they are. I myself claim to be the original and only discoverer of these inner satellites, *Ariel* and *Umbriel*, single-handed and unassisted, without coadjutor, rival, or competitor.

I enter upon this self-vindication most reluctantly, and even painfully—for egotism is essentially ungraceful—but I have no choice. And, in what I have to say, I desire especially to guard myself from any imputation of a desire to detract in the smallest degree from the transcendent merits of the late Sir William Herschel, as an Astronomer and practical observer; I may add also, as a Mechanic and Philosopher; none more willing than I to yield to him admiration and eulogy. His fame is unassailable, and needs no questionable support. Therefore, it is unwise for any of his friends, among whom I desire to range myself, to attempt to add to his achievements anything which stern, uncompromising, and stubborn truth can possibly invalidate.

In a Paper read before the Royal Society on February 15, 1787, and published in their *Transactions* for that year, Sir William Herschel announces his discovery of the first two satellites of the planet *Uranus;* which have since received the names of *Titania* and *Oberon*, and which he uniformly in his subsequent observations designates First and Second. He gives at page 128 a drawing of them accompanying the Planet. They were first seen together on January 11, and were fully corroborated on February 11. The whole paper occupies only four pages, and challenges the unqualified assent of every reader by its precision and ample detail.

In the volume for 1798 is a much longer Paper, by Sir William, read December 14, 1797, and extending from page 47 to page 79, in which he announces the "Discovery of Four additional Satellites." The observations given are very numerous, extending from 1782 to 1796; and towards the conclusion of his paper, namely, at page 66, he has the following paragraph:—"In such delicate observations as these of the additional satellites, there may possibly arise some doubts with those who are very scrupulous; but, as I have been much in the habit of seeing very small and dim objects, I have not been detained from publishing these observations sooner, on account of the least uncertainty about the existence of these satellites," etc. Then follows—"The arrangement of the four new and the two old satellites together will be thus:—

First satellite, the interior one of Jan. 18, 1790.
Second satellite, the nearest old one of Jan. 11, 1787.
Third satellite, the intermediate one of Mar. 26, 1794.
Fourth satellite, the farthest old one of Jan. 11, 1787.
Fifth satellite, the exterior one of Feb. 9, 1790.
Sixth satellite, the most distant one of Feb. 28, 1794."

1874MNRAS..35...22L

At a later page Sir William assigns to them the following periods of revolution:—

	d	h	m
First	5	21	25
Second	8	16	56
Third	10	23	2
Fourth	13	11	6
Fifth	38	1	48
Sixth	107	16	39

In the *Philosophical Transactions* for 1815 is a further Paper, read June 8 in the same year—"On the Satellites of the Georgian Planet"—extending from page 293 to page 362, in which Sir William discusses and comments upon the observations of the former paper, with an elaborate description of what he calls a "method of identification" of the satellites; and after a great variety of remark and re-quoting of his former observations, without many new ones, he concludes, without throwing any doubt upon the reality of his former supposed discovery of four additional satellites. Yet after the lapse of more than sixty years, during which the most competent existing telescopes, including the recently-erected twenty-six-inch refractor of the Washington Observatory, have been employed in the search, *not one of these four has ever been verified.* Therefore I would ask—Is it admissible to claim for Sir William Herschel "that he was in truth the discoverer of *Ariel* and *Umbriel*"—satellites nearer to *Uranus* than the nearest of Herschel's supposed four, and therefore more difficult to see—while all the four which he thought he had discovered are now proved to have been only imaginary? After very careful and repeated examination of the whole series of published observations, I am forced to conclude that they are much too loose and indeterminate to prove the existence of any new satellite. In fact, the means at Sir William Herschel's disposal did not suffice to reveal to him the existence of these most minute bodies.

In contrast to these voluminous papers, may I be pardoned for yielding to the temptation of referring the reader to my own account of my discovery of these inner Satellites, at page 247 of Vol. XI. of the *Monthly Notices*, where are four diagrams of the positions of the Satellites on October 24, 28, and 30, and November 2, 1851, which are sufficient in themselves to establish the discovery. I do not, however, myself date it earlier than November 13, when in my Paper, at page 15 of Vol. XII. I give the approximate periods of 2·506 days for *Ariel*, and 4·150 days for *Umbriel*—sufficiently near, I venture to say, at the then elapsed time, to the now recognised periods. It is needful to add, that there are two errata in the diagrams at page 247, which are respectively corrected at page 255 of Vol. XI., and in the middle of page 17 of Vol. XII. One

1874MNRAS..35...22L

word more respecting this discovery. No subsequent observations have, I believe, ever thrown the smallest doubt upon its reality and correctness—though I have waited a quarter of a century for the construction of a telescope sufficiently perfect and powerful to deal with these excessively minute objects, and to give that triumphant corroboration which Professor Newcomb's observations now afford.

This letter is already longer than I desired, and yet, as Professor Holden has attempted to establish his claim for Herschel's discovery by backward calculations of the places of *Ariel* and *Umbriel* at the several epochs quoted, I cannot refrain from some remarks upon one or two of his verifications. Take, for, instance the first he names — 1790, January 18 — when Professor Holden conceives Herschel saw *Umbriel*. Herschel's exact words are, page 314 of the 1815 vol.—" 1790, Jan. 18th, $9^h\ 32^m$. There is a supposed third satellite about 2 diameters of the planet, following, extremely faint, and only seen by glimpses. $1^h\ 6^m$ after I could not perceive it; a fourth is about 70° n.p." On the following day, the 19th, Herschel made two observations for verification of the supposed fourth, and satisfied himself that it was a star. He appears, however, to have made no search after the supposed third. Is it not to be therefore presumed that he did not believe in the observation himself, or surely he would have taken especial pains to verify so interesting an object as a supposed satellite within two diameters of the planet? Moreover, referring to this observation, originally recorded in the 1798 vol., he says in his 1815 paper, many years after—" It is very strange that the third supposed satellite should not have been attended to, when two observations are given to prove that the supposed fourth was not a satellite." Further, at page 358 he says—" The supposed interior satellite observed Jan. 18, 1790, could not be a star whose existence was doubtful, as it had light enough for an estimation of its distance in diameters of the planet," [a statement quite irreconcileable with the terms of the original record, stating that it was "extremely faint, and only seen by glimpses," and one hour after could not be seen at all]—" its absence, however, not having been noticed the 19th January, although great attention has been shown to ascertain the sidereal nature of another supposed satellite, observed at the same time with the former, leaves the observation of the 18th unsupported." Yet this observation which Herschel himself in 1815 says is "unsupported," is one of the three which Professor Holden states "have been verified fully." Again, "1798, Feb. 15, $12^h\ 13^m$, a supposed fifth satellite was observed"—afterwards proved to be really *Oberon*. A due familiarity with the appearances of these very faint and dim points would have rendered such a mistake as this well-nigh impossible.

Towards the close of his paper, Professor Holden says, " Lassell estimates that *Oberon* and *Titania* are twice as bright intrinsically as either of the inner satellites, and this estimate is

1874MNRAS..35...22L

probably not too high." On this point Herschel remarks that the old satellites generally disappeared when they came within about 22″ of the planet—his exact words are "he could seldom see" *Oberon* nearer to the planet than 23″·6 and *Titania* "was usually invisible" at less than 18″·1. And he gives a table at page 76 of the first Paper, of dates on which the old satellites were invisible, and their calculated distances at those times, namely:

			″				″
1791	Feb. 22	First	21·8	1792	Feb. 21	Second	23·3
	Dec. 19		16·9	1794	Mar. 17		20·7
1792	Mar. 15		18·4		Mar. 23		17·9
1794	Mar. 4		18·5				
	Mar. 17		17·0				

and adds the following notes:

"March 15, 1792. I cannot see the first satellite with 300, nor with 480, nor with 600."

"March 4, 1794, the first satellite could not be seen." And he says further, "In very uncommon and beautiful nights the first has once been seen at 13″·8 and the second at 17″·3, but no time have they been visible when nearer the planet." Therefore, estimating these inner satellites at only half the brightness of the old ones and their *greatest elongations* being only about 14″ and 19″ respectively—is there the smallest reason to suppose that Herschel could ever have obtained a glimpse of them?

With respect to the backward calculations, even supposing that unquestionable data exist for carrying them with sufficient accuracy to so remote an epoch (which I am by no means sure of)—when such a latitude is given to Herschel's measures, as to accommodate them with presumed errors of position-angle amounting to 15°, 23°, and 34°, and a surmise that in one case at least, the angle had been written down 180° wrong—taking the choice, moreover, of whichever of the two satellites might suit the calculation best—is it any wonder if some observations should be picked out of such a mass, and, with such license, made corroborative?

Therefore I cannot think that Professor Holden will carry the majority of his readers with him when he says, "that of the four cases where Herschel supposed he saw the interior satellites, three have been verified fully, and a reasonable suspicion exists that the fourth may have been likewise a veritable observation of *Umbriel*." But my letter is already too long to permit me to enter into a discussion of these four cases, which would indeed lead me to an opposite conclusion.

I will only make one further quotation, and it is really painful for me to be compelled in self-vindication to disinter observations which had better been left to slumber for ever. In reference to observations of February 9, 1790, at page 64, Herschel says, "A new satellite was seen in a line with the planet and second

1874MNRAS..35...27L

satellite. As it was in a line continued from the planet through the second satellite, its orbit must necessarily be of a greater dimension than that of the second, I shall therefore put it down as an exterior satellite." Most likely this satellite also was seen amongst the supposed satellites south of the planet, March 27, 1794, where we find mention made "of some others south, at a good distance." In that case this will make a second observation. Will any astronomer be found to call this *evidence?*

Finally, it is obvious that my subsequent discovery can neither add to nor take from the intrinsic value of Sir William Herschel's observations; and therefore if they do really suffice to reveal the existence of *Umbriel* and *Ariel*, and show their places in the Uranian system, why did not Sir William Herschel, in the long interval between his first and second Paper, logically deduce and prove therefrom their existence? And in his default, why has not some other hand done the work during the half century previous to my discovery? Really the insufficiency of the observations forces itself upon the mind like an axiom—no reasoning can make it plainer.

I remain, dear Sir, yours truly,
WM. LASSELL.

Ray Lodge, Maidenhead,
1874. *November* 27.

Observations of Iapetus. By W. Lassell, Esq.

At the request of my friend, Mr. Marth, to whom we are indebted for the published Ephemerides of the Satellites of the planet *Saturn*, I have observed the late superior and inferior conjunctions of *Iapetus*, although the circumstances on both occasions were very unfavourable.

1874, September 24.—At about 20^h 45^m Sidereal Time the planet could be observed for a few minutes only, the sky suddenly clouding before even a diagram could be made. The Satellite was much fainter than I expected. It was situated north of the preceding arm of the ring, perhaps about 40 or 45″ from its axis. *Rhea* was near conjunction, south of the same arm, and I noticed that *Iapetus* was much fainter than *Rhea*.

September 25.—At 21^h 8^m Sidereal Time, *Iapetus*, by a mean of several micrometer measures, was 53″·0 north of the major axis of the ring, and exactly in a line with a tangent to the planet's following limb, perpendicular to that axis. The air was muddy and disturbed.

November 2.—The sky was perfectly cloudy. I need